

approximately, the mounting of the Equatoreal was firmly attached to it by means of bolts, screws, and clamps.

The mounting, which, as well as the telescope, was made by Cooke & Sons, of York, consists of a polar axis carrying a single hour-circle, and a cradle-piece so constructed that the telescope may be separately affixed or withdrawn at pleasure. The cradle-piece forms an appendage to the declination-axis, at right angles to the former, which carries the declination-circle.

The telescope, then, being placed on its mounting, both were approximately set in position, *i.e.* the polar axis in the direction of the pole and the axis of the telescope with its plane of motion coincident with the plane of the meridian. This being effected, the telescope was pointed to a star, and then the mounting was moved about on its bearings by means of foot-screws, and round a vertical screw, forming an axis, until the declination-circle indicated the true North Polar Distance of the star when in the centre of the field of view. A marine chronometer, tested to Greenwich Mean Time by means of a Dipleidoscope, was employed to determine the Right Ascension of the meridian of the place at a given instant, and thence, by subtraction of the star's Right Ascension, its Hour-angle was found for the same instant. For convenience, the instant selected was the commencement of some hour on a common watch, its error being compared with the standard chronometer, and thus the Hour-angle of the star at any following instant was obtained at a glance by adding the minutes and seconds elapsed on the watch since the commencement of the hour nearest to it.

This done, two coordinates were obtainable for the same instant, *viz.* the true North Polar Distance of the star, and its true Hour-angle; and thus, by moving once more the mounting on its bearings, the hour-circle was made to indicate the true Hour-angle, and then the position was made permanent by the screws.

The adjustment was found so far satisfactory that planets and stars could be made visible in the day time by its means, which is a competent test of its accuracy.

Note on the Difference of Variation of Gravity at Revel and St. Petersburg; and on Grischow's Pendulum Observations at other Stations. By Major J. Herschel, R.E., F.R.S.

The difference to which I wish to draw attention is that exhibited on comparing the observations at the two stations named, by Grischow* in 1757, and by Sawitsch in 1865.

As such a comparison will seem at first sight derogatory to the modern observation, I will give my reasons for thinking that the more ancient are not devoid of reliance.

* *St. Petersburg Acad. Sci. Novi Comm.* vii., pp. 447-451.

Grischow employed two pendulums, one of which had seen service with La Condamine (who had had it made) in Peru, and the other with La Caille at the Cape of Good Hope. The former was swung in connection with machinery which counted its oscillations; the other was swung free, and its rate was determined by coincidences with the pendulum of an astronomical clock, every twelve or thirteen minutes.

Grischow observed these two at five stations, and were it not for the unaccountable difference between his results and those of Sawitsch, I venture to say that they should enlist immediate confidence on the sole ground of their concurring testimony.

I do not propose to go into any details of reduction here; but in order to show the grounds of this confidence, I give the vibration-numbers, reduced to 13°·3 R. or 62° F., which is about the average temperature of the whole series. The observations at Arensburg extended over nearly a twelvemonth, and supply good temperature factors, viz. 1·1 and 0·7 respectively for 1° R. For the purpose of comparison it is necessary to reduce the one to the unit of the other, or both to a common unit. I prefer the latter method, and adopt the Equatoreal Seconds Pendulum. By means which it is not necessary to describe, I estimate that the one (A) would make 98753·0, and the other (B) 86350·0 vibrations, in air, at 13°·3 R., at the Equator, or near it; and these are the numbers I use to reduce to the terms of an Equatoreal Seconds Pendulum vibrating 86400.

The following short table exhibits the data* and comparison. (I have added La Caille's observations at Paris as quoted by Grischow,* but reduced to 13°·3 R.):—

	A	B	A	B	A - B
St. Petersburg	98950·1	86519·9	86568·0	86570·0	-2·0
Revel	941·8	510·2	560·8	560·3	+0·5
Arensburg	937·4	504·9	556·0	555·0	+1·9
Pernau	940·6	509·4	559·7	559·5	+0·4
Dorpat	939·6	510·6	558·9	560·7	-1·8
Paris	900·5	453·1	524·7	503·2	...

There is no selection of observations here. The figures represent the whole of Grischow's published observations; and I think that on the face of it they are entitled to confidence. I exhibit them solely to justify the inquiry which I shall now proceed to make being listened to. I base nothing upon them as yet.

Returning now to the unreduced observations at St. Petersburg and Revel, these are:

	At St. Pet.	By A	At 16·0 R.	By B	At 17·6 R.
„ Revel	„	941·8	„ 13·3 „	„ 510·4	„ 13 „
Diff.		+5·3	+2·7	+6·5	+4·6

* See also *St. Petersb. Acad. Sci. Nova Acta*, vii., p. 215 et seq.

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or with the temperature factors 1.1 and 0.7 respectively.

St. Pet. — Revel + 8.3 and + 9.7

which, when reduced to seconds, become

+ 7.3 and + 9.7

agreeing of course with the former reduction.

Now Sawitsch's observations, as last published (see *Monthly Notices* for March 1879)—observations which I am unwilling even to seem to question—give the following:

Length of seconds pendulum at St. Petersburg	441.0254
" " at Revel	441.0125

This difference corresponds to a change of Vibration-number of an Equatoreal Seconds Pendulum

$$= + \frac{1}{2} \frac{.0129}{441.0} \cdot 86570 = + 1.3 \text{ sec.}$$

I am quite unable to discover any reasonable explanation of the discrepancy. Naturally Sawitsch's has much the higher probability; and I have therefore examined the older observations carefully. I argued thus: Grischow's results are with entirely different pendulums, it is true; but do they depend on the same clock rates? Yes. Then the clock rate must be in error. In effect, I at once found an error of 10 sec. (see *St. Petersb. Acad. Sci. Novi Comm.*, vii., p. 451, l. 5) in the mean daily rate of the clock. But this proved to be only a misprint: the more correct rate had been used. Nor am I able to believe readily in the clock rate being wrong. The swings of A extend continuously over many days at both stations, the clock is rated by transits daily of the Sun and stars, and an undetected error of 6 or 8 seconds a day is scarcely conceivable.

I submit the difficulty therefore for the scrutiny of others, with the hope that some solution will be found.

A like difficulty attends—but in a less degree—the results at Dorpat. To show this I will suppose that my table above is accepted, subject to a possible constant but unknown correction to each of the second pair of columns, to allow for the provisional character of the adopted equatoreal numbers. I now show the same figures at four stations, of which the Local-numbers—by which term (with capitals) I denote the Vibration-numbers which we otherwise know, with more or less certainty, would be shown by an Equatoreal Seconds Pendulum—are annexed for comparison.

		A	B	Mean.	Loc. No.
1757	St. Pet. (Grishow)	86568.0	86570.0	86569.0	86569.5
1757	Revel ..	60.9	60.3	60.6	68.2
1758	Dorpat ..	58.9	60.7	59.8	64.0
1756	Paris (La Caille)	24.7	...	24.7	24.2

To guard against any suspicion that these figures—the very remarkable agreement of which with each other in certain respects, and equally remarkable disagreement in others, is patent—are the result of discriminative selection (a kind of survival of the fittest, in fact), I beg to say that they are the direct consequence of adopting the Equatoreal-numbers 98758.0 and 86350.0. Each pendulum *had* an equatoreal vibration number which was not known. We have a right to assume any number for any pendulum, in any given condition, which will best satisfy the observations. I propose these, and show how well they fit the observations. That is evidence in their favour. If they are right they are a key to help to discriminate and to indicate facts. Thus it appears, beyond any doubt, that the condition of B, when in Grischow's hands, was not the same as when in La Caille's; and the Vibration-number is accordingly dropped, as not belonging to the differential series.

I have gone further in this examination than I intended, in the hope of finding other reasons, in the discordance *inter se* of the results, for their non-appearance in any collection of pendulum observations. That the Revel and Dorpat results are now apparently irreconcilable with the more recent results of 108 years later is true; but was this foreseen? And is there even now the *smallest* ground for doubting the Arensburg value?

Calcutta, May 24, 1879.

Note on the Semi-diameter of the Moon.

By E. Neison, Esq.

The results obtained by Professor Pritchard from the measurement of the photographs of the Moon taken at Oxford, and given in the *Monthly Notices* for June, p. 447, are of considerable interest. Unfortunately, as Professor Pritchard gives no details of the method he employed, beyond stating that it is very similar to that employed by Wichmann (*Ast. Nach.*, 1847), it is impossible to say how far he has succeeded in eliminating the systematic errors which are incidental to this work. Professor Pritchard makes the semi-diameter of the Moon equal to

$$15 \quad 34'175.$$

From a careful discussion of nearly eleven hundred observations made with apertures of different sizes, at the Observatories of Greenwich, Oxford (Radcliffe), and Washington, I deduced the value

$$15 \quad 33'37 + 4'10 (1 + 0.70 \times \text{aperture in inches}).$$